Evaluating the Impact of Innovation Incentives: Evidence from an Unexpected Shortage of Funds[*]

Guido de Blasio,[◊] Davide Fantino,[•] and Guido Pellegrini[♦]

THIS VERSION: JANUARY 2010

Abstract

To evaluate the effect of an R&D subsidy one needs to know what the subsidized firms would have done without the incentive. This paper studies an Italian program of subsidies to stimulate the applied development of innovations. It exploits a discontinuity in program financing due to an unexpected shortage of public money. To identify the effect of the program the study implements a regression discontinuity design and compares firms that applied before and after the shortage took place. Results indicate that the program was not effective in stimulating innovative investments.
Keywords: R&D, public policy, evaluation
JEL Classification: O32, O38

[*] We are grateful to the participants in the Bank of Italy "Seminario di analisi economica territoriale," Rome 2009. We are deeply indebted with the Italian Ministry for Economic Development, which provided us with the official archive of the Fund for Technological Innovations. Usual disclaimers apply.
[◊] Bank of Italy, Structural Economic Analysis Dept.
[•] Bank of Italy, Turin Branch, Regional Economic Research Unit.
[♦] University of Rome "La Sapienza".

1. Introduction

Stimulation of innovation activity is considered a very important task for policymakers[1]. The theoretical case for public intervention to stimulate R&D is clear. As the social returns from innovations are usually greater than the private ones, the resources allocated by private firms are smaller than those required by the social optimum (Arrow, 1962). Notwithstanding this sound rationale, the evidence on the effectiveness of the subsidies is highly controversial. For instance, David, Hall and Toole (2000) discusses the findings of forty years of empirical studies and concludes that there is no conclusive support in favor of public support. The inconclusive evidence also depends on the intrinsic difficulties of the evaluation exercise. Evaluating the effects of government sponsored projects, one has to face the question of what would have happened without the subsidies.

What would the firm have spent on R&D, had it not received the subsidy? On one hand, a financially *constrained* firm would have spent less money. As such a firm cannot finance the whole project, without the subsidy – that lowers the private share of financing – the project would not have been implemented[2]. On the other hand, a financially *unconstrained* firm would have spent an higher amount of private money had it not received the subsidy. As such a firm can finance the project from external or internal funds, the subsidy is in fact superfluous (it crowds out private R&D expenditures). Note that the subsidy comes with a cost for the firm that is substantially lower than that of alternative sources of financing. Therefore, both constrained and unconstrained firms will ask for the subsidy. Given the difficulties to grant a subsidy conditional of being constrained, there is no guarantee on the effectiveness of the program. It can only be evaluated ex-post.

In principle, the 'gold practice' to evaluate effectiveness is randomization: the agency in charge of the program identifies a group of 'potential beneficiaries' and randomly awards subsidies within this group, meaning that the probability of receiving the subsidy is the same for all members of this group. Unfortunately, randomization schemes are almost never implemented[3]. Therefore to evaluate effectiveness, the researcher has to rely on identification strategies that aim at reproducing as closely as possible the fundamental feature of a random design (see the vast literature on program evaluation; for instance, Imbens and Rubin, forthcoming). As underscored by Duflo et al. (2007), because of exogenous events sometimes elements of randomization are introduced into programs not designed at the beginning as random schemes. This study elaborates on one of these circumstances.

This paper evaluates an Italian program of subsidies for technological innovation (Fund for Technological Innovation; FTI or Fund, thereafter), which was *not* designed through a random scheme. The FTI allocated the subsidies among the applying firms by selecting the research proposals considered more promising by a technical committee. However, at one point in time the *financing* of the program was unexpectedly interrupted due to a shortage of public money. At the same time, however, firms were still allowed to apply

[1] This is particularly true in Italy, where the amount of private resources dedicated to R&D (in 2000, it was 0.5 percent of GDP) compares poorly with that of countries with a similar stage of development.
[2] Compared to more traditional investments, for R&D the likelihood that profitable projects do not find adequate financing could be magnified. Indeed, R&D projects have an higher variance of success probabilities. Thus, the returns might not be clearly identifiable and this could be particularly true for non-specialist external financier, as commercial banks.
[3] Admittedly, it is not easy to understand why this is the case. For instance, Jafee (2002, p. 28) writes. "I remain personally puzzled as to why it is okay to randomize when people's lives are the stake (drug trials), but not when research money is at the stake."

for the subsidies, as there was a hope that the problems could have been soon resolved. Indeed, while the possibility to apply was detached 10 months after, the funding problems were resolved only 5 years later. Our strategy will basically compare firms that received the subsidies before the shortage with firms that applied in the subsequent 10-month period and were not assessed by the committee for 5 years. Thus, we contrast applying firms that passed the assessment (treated group) with applying firms that did not go trough the assessment process because of the suspension (control group). We argue that being in one of the two groups was essentially a matter of luck, and that this was particularly true for the firms that applied just around the day in which the interruption of the financing occurred. Therefore, the unexpected shortage of funds allows us to empirically implement a regression discontinuity design (RDD) that uses the date of the application as the *forcing variable* and the day in which the shortage occurred as the *cutoff*.

As for the results, we find no evidence of effectiveness. Compared to the suspended-assessment firms, the subsidized firms do not invest more in either tangible or intangible assets. Therefore, in the experience of the FTI public funding just substitutes for private funding. This result is highly robust to a number of sensitivity checks. While the effects of the program on sales, profitability and financial conditions of the firms are also negligible, we find a positive impact on the overall size of the balance sheet, which suggests that money saved on R&D was spent on alternative assets. Finally, the effectiveness of the program is not higher for firms that typically face an higher likelihood of being rationed by private lenders (such as SMEs or firms with high borrowing costs).

The remaining of the paper is organized as follows. Section 2 illustrates the characteristics of the policy intervention. Section 3 describes the data and sketches the methodology. Section 4 provides the results. Section 5 concludes.

2. The Fund for Technological Innovation

Main features. – Started in 2001[4], the FTI has the purpose of "stimulating the applied development of innovations through subsidies to the R&D activity of firms." The functioning of the Fund is similar to that of other commercial research-grant programs, widely implemented all over the world. A high-level executive agency (in our case, the Ministry for Economic Development, MED) is in charge of the program: it establishes the rules of the game. Firms apply for this money by writing and submitting substantive R&D proposals[5]. A technical committee organized by the agency meets, reviews the proposals and provides a judgment about their merits. On the basis of this judgment, the MED decides whether to grant the subsidy or not. In the case of rejection, it explains the committee's motives. There is no deadline for the submission and applications are evaluated one-by-one following the chronological order of receipt.

[4] To be sure, an ancestor of the Fund was around since 1982. In 2001, however, the FTI has been reorganized along the lines explained in the text.

[5] The applications are preliminary assessed by private banks, which have to express a view on the economic and financial soundness of the applicant firm and the project. The report of this assessment is sent to the MED, along with the R&D proposal. Then the committee considers both the proposal and the preliminary report to express its judgment. The bank assessment is quite uninformative. As explained by an official of the MED involved in the implementation of the FTI, the bank assessment is very favorable almost always.

Note that the focus of the Fund is on applied innovations. While the R&D proposal might include both a research component and a development component[6], the area of responsibility of the Fund refers only to the latter. In particular, if the research costs are prevailing over the development costs, the application is dealt within a different facility by the Ministry of University and Research[7], instead of the MED.

The overall amount of subsidy[8] is equal to the upper bound allowed by the EU regulation, which is 50 percent for research costs and 25 percent for development costs. As the projects managed by the MED have at least a 50,1 percent share of development costs, they receive a subsidy between 25 and 37,5 percent. The subsidy can be augmented up to an overall additional 25 percent in the following cases: SMEs[9], firms located in underdeveloped areas (defined according to the Objectives 1 and 2 of the EU regional policies), projects included in the objectives of the EU Research Framework Programs, projects in cooperation with other firms or public research organizations. The stated cost of the project might include expenditures for labor, machinery, consulting, general and consumption costs, feasibility studies and research center organization. As for the timing of the investments, the FTI envisages that it must start between 12 months before and 6 months after the date of the application. The project must be concluded between 18 and 48 months from the date of the application. A 12-month extension for the completion of the project can however be requested by the firm[10].

The unexpected shortage. – The Fund initiated its operations the 27th of Oct 2001 (see Fig. 1). From that day through the 17th of March 2002 it worked smoothly. The 18th of March 2002 the financing of the program was unexpectedly interrupted. The MED found itself constrained in its current availability of resources, given that the allocations were exhausted and the Treasury did not transfer to the MED additional provisions. At the same time, however, the MED left the firms free to continue to apply for the subsidies: there was a hope that the financing problems could have been soon resolved. Indeed, the possibility for firms to apply was detached the 13th of January 2003, as the public finance problems turned out to be more severe. As matter of fact, only at the end of 2007 the MED was in a position to reconsider the 1,242 R&D proposals by the firms that applied during the period 03/18/2002-01/13/2003. In particular, at that time these firms were notified that the MED was ready to start the committee's assessment process.

[Figure 1]

[6] According to OECD (2002), "research is experimental or theoretical work undertaken primarily to acquire new knowledge of the underlying foundation of phenomena and observable facts. Development is systematic work, drawing on existing knowledge gained from research and/or practical experience, which is directed to producing new materials, products or devices, to installing new processes, systems and services, or to improving substantially those already produced or installed".

[7] The Ministry of University and Research is in charge of the Fund for Support to Research, oriented to stimulate the research component of R&D. An evaluation of the effectiveness of subsidies for projects with a prevailing research component can be found in Merito et al. (2008).

[8] The subsidy is a combination of a concessional loan (which covers 60 percent of the subsidy and charges an interest rate that amounts to 1/5 of the market rate) and a grant (for the remaining part). The maximum length of the reimbursement plan of the loan is ten years, plus a grace period during the execution of the project.

[9] The MED uses the following criteria to identify SMEs: the number of employees must be lesser than 250, the firm must be independent and either the overall annual sales revenue must be less than 40 million euros or the overall assets must be less than 27 million euros in the last fiscal year.

[10] The subsidies are disbursed in *tranches*, the financial plan may include up to five installments. The payment is made by 60 days after the certification by the firm of the covered expenses. SMEs, however, can ask an upfront payment for the initial installments.

3. Data and Methodology

Sources. – Our main data source is the MED official archive of the FTI program. To ensure greater homogeneity we only consider manufacturing firms (around 80 percent of the firms in the MED archive). The version of the archive released to us records: i) among the firms that have applied from 10/27/2001 to 03/17/2002 only those that have received the subsidy[11]; ii) all the firms applying from 03/18/2002 to 01/13/2003, whose assessment was suspended. The archive includes for each firm the following information: name, address, fiscal code, amount of the R&D expenditure programmed, amount of the subsidy (with a breakdown between grant and concessional loan), share of the development component over the total R&D expenditure. Only for the subsidized projects it makes available both the date of the start of the project and that of the conclusion. We link the FTI archive with the Cerved datasets of financial statements to reconstruct an uninterrupted sample from 1999 to 2007. In the linking procedure, firm identifier (fiscal codes) misprints, the unavailability of the balance-sheet data over the entire period, and standard data cleaning[12] reduce the sample to 751 firms (329 of which subsidized).

Outcome variable. – An important issue is where R&D expenditures end up in the financial statements. The point is that an R&D project might include a number of items (labor and services costs, physical capital, intangible capital), which could be registered in either the *Balance sheet* or the *Profit and loss statement*, according to the preference of the management. Luckily enough, Italian accounting rules (see: Pisoni et al., 2009) envisage that for the projects in which the development share prevails over the research share, all the costs (even those with a single-year utility) have to be capitalized and shown into the Balance sheet as tangible or intangible investments. Therefore, our main outcome variable is elected to be total investments (over pre-dated capital) as documented in the Balance sheet.

Timing of the investments. – We compare subsidized firms with firms that applied from 3/18/2002 on, when the assessment process was suspended. As explained above, FIT rules envisage that the project must start between 12 months before and 6 months after the date of the application and be concluded between 18 and 48 month from the date of the application (while firms can ask and receive a one-year extension for completion). The MED archive includes the actual timing of the subsidized investments. For the comparison, we need to figure out what the investment time profile of the suspended-assessment firms would have been had they decided to carry out the project anyway.

[Fig. 2]

If we had no data on the actual beginning and completion times, we could have inferred (Fig. 2) that subsidized firms – that applied from 10/27/2001 to 3/17/2002 – could have started their project anytime

[11] Data on rejected firms were not released because of confidentiality. To be sure, the Italian law for the safeguard of the privacy envisages that the names of the firms that receive public money have to be disclosed. Such a provision does not apply for firms that asked for the money without receiving it. Indeed, for rejected applicants the MED feared that disclosure could signal a low quality of their investment prospects. As no judgment was conducted for the firms that apply in the period 03/18/2002 - 01/13/2003, the names of these firms were released without difficulties.

[12] To remove outliers, we dropped the firms in the 1st and the 99th percentile of the distribution outcome variable. Firms experiencing M&As during the period were also excluded.

between 10/27/2000 and 9/17/2002, and completed it between 4/27/2003 (for a project submitted on 10/27/2001 and completed in 18 months) and 3/17/2007 (for a 48-month project that was submitted on 3/17/2002 and extended for additional 12 months). As we have data for the treated firms on the actual timings (Fig. 3), we can check the extent to which the hypothetical timings approximate the actual timings. We find that the approximation is very close: all the subsidized firms began their projects by the end of 2002, while no project was initiated before the 1st of January 2001[13]; in the years 2003, 2004, 2005 an increasing share of firms completed the project. By the end of 2006 all firms concluded it.

Suppose now that the suspended-assessment firms – that applied between 3/18/2002 and 1/13/2003 – carried out the project anyway. What should we have observed? For these firms the hypothetical time profile of the investments can be inferred similarly to what we have done for the subsidized firms pretending we had no data. Therefore, these firms would have started their project anytime between 3/18/2001 and 7/13/2003 and completed it anytime between 9/18/2003 (for a project submitted on 3/18/2002 and completed in 18 months) and 13/1/2008 (for a 48-month project submitted on 1/13/2003 and extended).

[Fig. 3]

As shown in Fig. 2, there is a substantial overlap between the investments time pattern of the treated firms and that of the control firms, had they implemented the project anyway. For both, the projects should have been substantially completed by the end of 2007. Therefore, by comparing cumulative investments over the 2001-2007 period between treated and suspended-assessment firms, we should be able to detect whether the subsidy made it possible some additional investments. By the same token, subsidized projects started before the counterfactual ones: at the end of 2006 while the bulk of the subsidized project was ended, some share of the counterfactual projects – if implemented – could have been in the process of being completed. In the empirical section we deal with the fact that subsidized firms might have started their project some months in advance compare to what could have done their counterparts by estimating the effect of the subsidies only for the firms that applies just few days before and few days after the day when the unexpected shortage occurred.

Note, however, that the 2001-2007 comparison could be upwardly biased because of time substitution (see: Bronzini and de Blasio, 2006). This occurs if some subsidized firms brought forward investment projects originally planned for some period later to take advantage of the incentives. As the suspended-assessment firms did not receive the incentive, they might have decided to implement their project according to the original timetable. The upward bias due to the potential of time substitution amounts to say that should we find a positive effect of the subsidies our results have to be deemed as inconclusive.

Missing data on rejected applicants. – As explained above, we do not have data on firms that applied before the day of the unexpected shortage whose applications were rejected. Therefore, we will basically compare firms that were successful in receiving a positive assessment by the technical committee with firms that did not go through the committee's assessment. As underscored by Lach (2002), government bureaucrats are

[13] While the Fund rules allow to be considered projects initiated before the date of application, only 5 percent of the subsidized projects was initiated before the application was submitted.

under strong pressure to avoid the appearance of wasting public funds and therefore tend to fund projects with higher success probabilities and clearly identifiable results. Indeed, it is very hard to believe that the assessment by the committee was systematically biased in favor of projects of lower quality. As a result, R&D projects of the treated firms might be better than those of the control firms, which include both those deemed as convincing and those that would have instead been rejected. It could then be possible that unsubsidized lower-quality projects were not implemented. This implies that there are reasons to believe that our results can be upwardly biased. Again, the upward bias due to the unavailability of data on rejected applicants means that should we find a positive effect of the subsidies our results have to be deemed as inconclusive.

Regression discontinuity design. – To estimate whether the subsidies made possible investments that otherwise would not have been undertaken, we implement a sharp regression discontinuity design (RDD). In this non-experimental setting the treatment is determined by whether an observed "forcing variable" exceeds a known cutoff point. We use the date of the application as the forcing variable and the day in which the shortage occurred as the cutoff. The main idea behind this research design (see: Thistlethwaite and Campbell, 1960; Angrist and Lavy, 1999; Black, 1999; and van der Klaauw, 2002) is that firms that applied just before the cutoff are good comparisons to those that applied just after the cutoff. This strategy is deemed preferable to other non-experimental methods because (see: Lee, 2008) if the applying firms are unable to precisely manipulate the forcing variable, the variation in treatment around the threshold is randomized as though from a randomized experiment (as if the firms had been randomly drawn just below or just above the threshold). An implication of the local randomized result is that RDD can be tested like randomized experiments. If the variation in the treatment near the threshold is approximately randomized, then it follows that all "baseline covariates" – all those variables determined prior to the realization of the forcing variable – should have the same distribution just above and just below the cutoff. To substantiate the empirical strategy, in the next Section we show that is extremely unlikely that the applying firms were indeed capable to manipulate the forcing variable and present a test for the absence of discontinuity in baseline characteristics around the threshold.

4. Empirical Evidence

Substantiating the identification strategy. – Lee and Lemieux (2009) explain that as long as firms exert some control over the forcing variable, which however is not a precise control, the conditions for the validity of an RDD are not violated. Note that it is very difficult to believe that our firms could have had a precise control over the forcing variable. While the possibility that the financing problems would have impacted on the functioning of the Fund could have permeated somehow out of the MED offices, the exact day in which the MED suspended the assessment was not known. Indeed, while the suspension applied for the applications received from the 18th of March onwards, it was notified to the firms only the 7th of May. Note also that at the time of the notification the MED made it clear that the problems could have been soon resolved, as firms were still allowed to apply for the subsidies. They did so for the subsequent 10 months; then this possibility was detached. Very likely, the applying firms realized at that point that the expectation of being assessed

soon was misplaced.[14] To corroborate the supposition that firms might have had (at most) an imprecise control over the forcing variable, Fig. 4 illustrates the daily number of submitted applications by application date. It is around 7 in the pre-shortage period and 4 in the post-shortage period and increases somewhat around the cutoff date (day 0), remaining however well balanced on the two sides. The only visible outlier (16 applications) refers to the first day in which applications started to be submitted (the 5th of November 2001). Note also that that the daily number of applications increased to some extent towards the end of 2002, that is before the chance of submitting applications was detached[15].

[Fig. 4]

We also formally test for the presence of a density discontinuity at the threshold by performing a McCrary test. This test, which is based on kernel local linear regressions of the log of the density of the forcing variable run on both sides of the threshold separately (see: McCrary, 2008), reveals that no discontinuity can be found.[16,17]

A key implication of the RDD framework (see: Lee and Lemieux, 2009) is that its validity can be tested by examining whether observed baseline covariates are locally balanced on either side of the cutoff. This evidence would substantiate the idea that the assignment of the treatment near the cutoff is approximately randomized. Table 1 presents for both treated and control firms means, standard deviations, and mean differences between the two groups for a number of baseline covariates referring to year 2000, which is the year before the program was announced (the year before realization of the forcing variable). Following previous literature, the table displays the following variables: net overall investments (over 1999 capital), net intangible investments (over 1999 intangible capital), (log of) sales, (log of) assets, long term debt (over assets), cash flow (over assets), average interest costs, ROA. Panel A considers the full sample of 751 firms. We find that the baseline covariates are roughly balanced on the two sides of the cutoff. However, average interest costs for treated firms are higher than for control firms. Panel B focuses on a sample of bandwidth (-90 days, +90 days) around the cutoff. As in Lalive (2008), the bandwidth is heuristically chosen to obtain a sample size of an half of the full sample. Differences in covariates are again overall insignificant with the only exception of average interests costs. As we checked, smaller bandwidths (we tried ±45-day and ±30-day) deliver very similar results.

[Table 1]

[14] In a randomized setting, potential treated are shortly notified about whether they are randomized in or out. In the FTI setting suspended-assessment firms realize that are not involved in the program with a delay ranging from few days (for firms that apply just before the suspension of the applications) to 10 months (for firms that apply just after the shortage took place).

[15] This fact might suggest some sorting occurring before the option of applying expired. Perhaps, the possibility that the financing problems could have been more severe than initially envisaged permeated and this might have pushed some firms to rush to applying. As it will be shown in the empirical section, our estimates are robust to methods that exploit only the information in a limited neighborhood of days around the cutoff. Therefore, potential sorting at the end of the period in which firms were allowed to apply does not seem to impact on our estimates.

[16] The jump in density at the threshold was estimated by using three different optimal bandwidths (15.5, 31 and 62 days, respectively). The point estimates were 1.13 (standard error 1.62), 1.61 (2.70), and -1.04 (1.17), respectively.

[17] We are aware that a density test may have low power if manipulation occurs on both sides of the cutoff. However, there are no reason why firms would have sorted after the day in which the shortage occurred.

As recognized by Lee and Lemieux (2009), with a large set of covariates some of the differences will be statistically significant by random chance. It is then useful to combine the multiple tests into a single test statistic to see if the data are consistent with random treatment around the cutoff. Table 2 presents the results we obtain by estimating by Seemingly Unrelated Regressions (SUR) where each equation (with a function form as in equation (1) below) represents a different baseline covariate. A χ^2 test for discontinuity gaps in all the equations being zero is highly supported by data for both the full sample and the ±90-day bandwidth subsample[18].

[Table 2]

Results. – Fig. 4 reports the cumulative (2001-2007) investments over pre-dated capital by the date of application. The evidence is based on the 329 subsidized firms (at the left of the threshold) and the 422 suspended-assessment firms (at the right of the threshold) of the full sample. We graph the mean of the outcome variable for each value of the discrete forcing variable (the date of the application). The figure superimposes the fit of a linear regression allowing for a discontinuity at the cutoff and linear trends in the forcing variable on both sides of the cutoff. From the figure it seems that there is only a minor jump in the outcome variable at the cutoff. The jump would indicate that the investment activity of the financed firms is even lower than that of the control firms.

[Figure 5]

We turn now to more formal measures of the effect of the subsidy. As explained above, the focus of RDD is on identifying the discontinuity in investments at the cutoff c. We start by using the following linear regression:

$$(1) \qquad I_i = \alpha_0 + \alpha_1 D_i + \beta_0 (X_i - c) + \beta_1 D_i (X_i - c) + \varepsilon_i$$

The parameter α_1 measures the average causal effect of the FIT subsidies on investments at the cutoff c. The parameters β_0 and β_1 capture the direct effects of the forcing variable X on the outcome I. The crucial issue in RDD estimation is the specification of the correlation between the outcome I and the forcing variable X. We propose two ways to assess whether the two-sided linear model specification (1) is appropriate. First, we parametrically evaluate the sensitivity of the results by augmenting the regression with quadratic and cubic terms in (X-c). Second, we move to non-parametric estimates (Pagan and Ullah, 1999) by running local linear regressions (Hahn et al., 2001) and estimating a triangular kernel (Fan and Gijbels, 1996). By relying only on outcomes from the firms that applied in a neighborhood of the cutoff, the non-parametric results also provide robustness with respect to the circumstance that subsidized firms started their project sometime in advance compared to what could have done their counterparts.

Table 3 presents the regression results on the effect of the FIT subsidy on investments. Column (1) reports an estimate that contrasts average 2001-2007 cumulative investments on both sides of the cutoff. Results

[18] For these regressions, we also performed a robustness analysis similar to that reported in Table 3 below, with no modifications for our results.

indicate that the investments of the subsidized firms are higher than those of the suspended-assessment counterparts (the estimate would amount to a 5 percent annual difference). However, the estimate is not statistically significant. The second column reports the results from the basic model of equation (1). We find that the estimated impact turns out now to be negative, remaining however highly insignificant. Columns (3) and (4) report the results from the quadratic and cubic specifications, respectively. Again, there seems to be no effect of the FIT incentive. Column (5) describes local linear regression results, where the model (1) is estimated over ±90-day bandwidth subsample. Results remain undisputed[19]. Column (6) turns to estimation of a triangle kernel. As suggested by Fan and Gijbels (1996), for boundary estimation a triangular kernel is more efficient than the more standard rectangular kernel, as the former puts more weight on the observations closer to the cutoff point. The ±12-day bandwidth is chosen using the rule of thumb procedure proposed by Silverman (1986). The estimated impact of the treatment is negative and non significant.

Column (7) adds to the baseline specification of Column (2) a number of covariates. We include a set of dummy variables for the location of the firm (at the region level) and a set of two-digit industry dummies. The effect at the cutoff is basically zero. Column (8) provides the estimate of the impact by comparing cumulative investments over the 2001-2006 period, instead of over the 2001-2007 one. This comparison is clearly biased in favor of finding a positive effect: at the end of 2006 while the bulk of the subsidized project was ended, some share of the counterfactual projects could have been in the process of being completed. Again, we find a negative albeit insignificant impact.

[Table 3]

Results so far document that there is no increase in total investments as a consequence of the receipt of the public funds. Note however that the outcome variable – total investments – reflects not only the R&D expenditures for the which the subsidy was granted or only requested, but also the additional investments that the firm has undertaken during the 2001-2007 period. For instance, suspended-application firms could have chosen to implement more traditional investments instead of the unsubsidized R&D investments and this could explain why the overall effect is zero. To check for this possibility Column (9) uses as a dependent variable the ratio of intangible investments over intangible capital. In contrast with overall investments, intangibles could be seen as more strictly related to R&D expenditures. As we fail to find any effect for the intangible investments as well (indeed we find a negative albeit insignificant impact), we conclude that substitution between R&D investments and less innovative investments seems not to be the reason behind our results.

Table 4 presents the result we obtain by using alternative outcomes. For the sake of brevity, we only present the estimates analogues to that of Table 2, Column (2). Coherently with the fact that firms get subsidies for projects that would have been undertaken even in absence of subsidy, we fail to find any significant effect on sales, financial conditions of the firm (long term debts over assets and cash flow over assets). We also find no effect on the average interest rate charged by external financiers and the ROA. Finally, we find a positive impact on the overall size of the balance sheet, which suggest that money saved on R&D might have been capitalized on alternative (non-investment) assets.

[19] Estimates are not sensitive to using smaller bandwidth.

[Table 4]

Table 5 displays the results we obtain by splitting the sample along some potentially interesting dimensions. For instance, economic reasoning suggests that the effectiveness of the subsidy should be greater for firms that face an higher likelihood of being rationed by private lenders, such as SMEs or firms with high borrowing costs (see: Guiso, 1998). As a matter of fact, we fail to find any sign of effectiveness even when we only consider these two categories of firms. Again, an argument often made by the practitioners is that R&D subsidies are usually wasteful, unless they can be targeted to firms that already have a sufficient know how in innovation activity. To check for this possibility, we estimate the impact of the program only for the subsample of firms with high immaterial assets in the balance sheet. Results are not in favor of the practitioner argument.

[Table 5]

5. Conclusion

Innovation is commonly called forth as one of the main engines of growth. Therefore, relevant policy fora at the national and international level[20] routinely highlight the role of public support to stimulate innovation. Beyond public declarations and legitimate hopes, however, little is known on the effectiveness of public money to foster private R&D expenditures. The reason is that to evaluate the effects of government sponsored program, one has to face a key, intrinsically difficult to answer, counterfactual question of what would have happened without the subsidies. In principle, a ready-to-implement method to provide a decisive answer to the key counterfactual question is available: it is randomization. In practice, this method is almost never implemented. For this reason, researchers have been struggling for long time on identification strategies that aim at reproducing as closely as possible the fundamental feature of a random design. Sometimes because of exogenous events some elements of randomization are introduced into programs that are not designed at the beginning as random schemes. This study elaborates on one of these circumstances: the unexpected shortage of funds that occurred with the Italian Fund for Technological Innovation. We compare firms that received the subsidies before the shortage with firms that applied after the shortage, whose assessment to receive the incentive was suspended. As being in one of the two groups was essentially a matter of luck – in particular for the firms that applied just around the day in which the shortage occurred – we implement a regression discontinuity design.

Our results point to a simple conclusion. There is no evidence of effectiveness whatsoever. Compare to suspended-assessment firms, subsidized firms do not invest more in either tangible or intangible assets. Basically, subsidized firms get subsidies for projects that would have been undertaken even in absence of the subsidy.

[20] For instance, R&D is one of the priorities of the EU Lisbon Strategy.

Fig. 1

Timeline of Applications

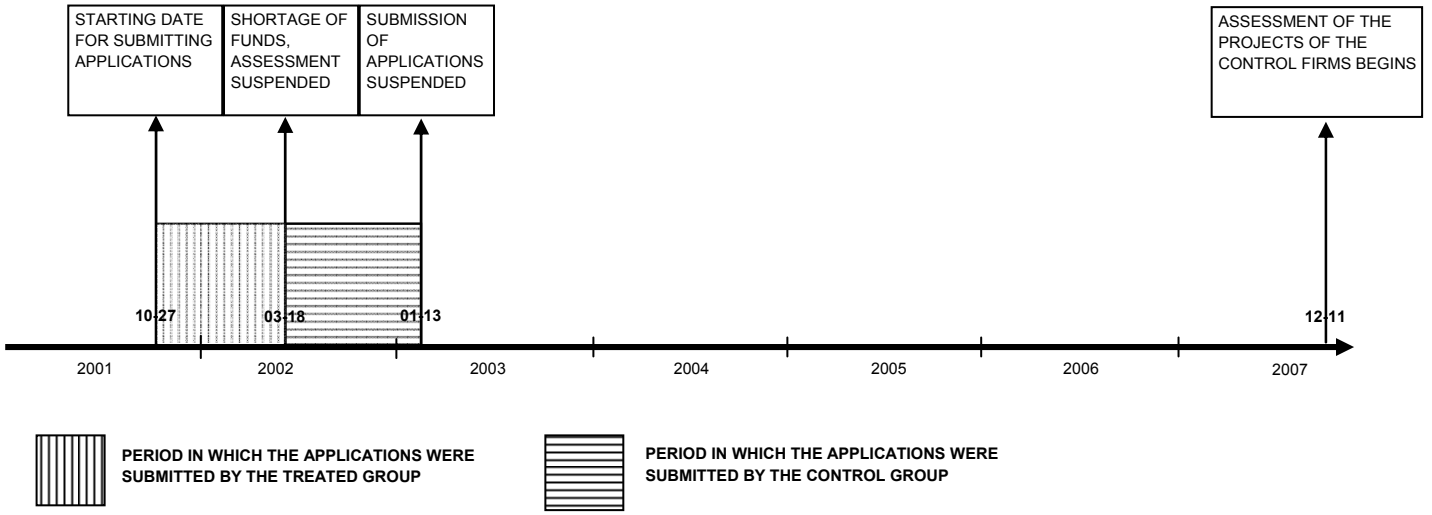


Fig. 2

Hypothetical Timeline of Investments

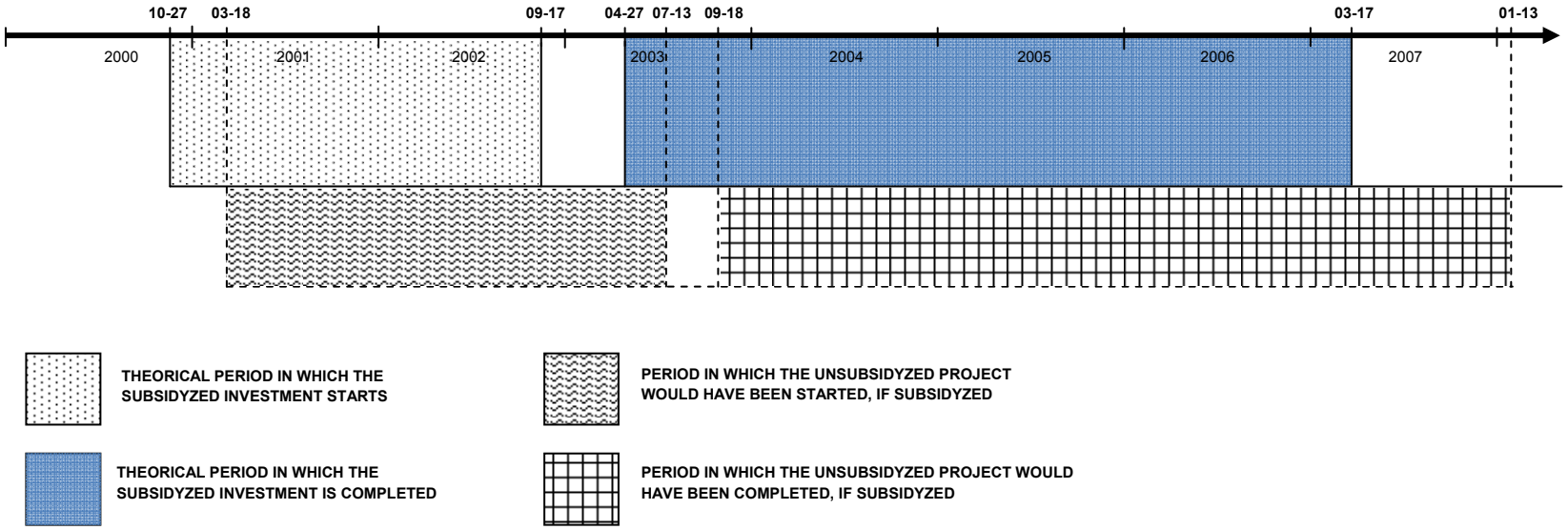
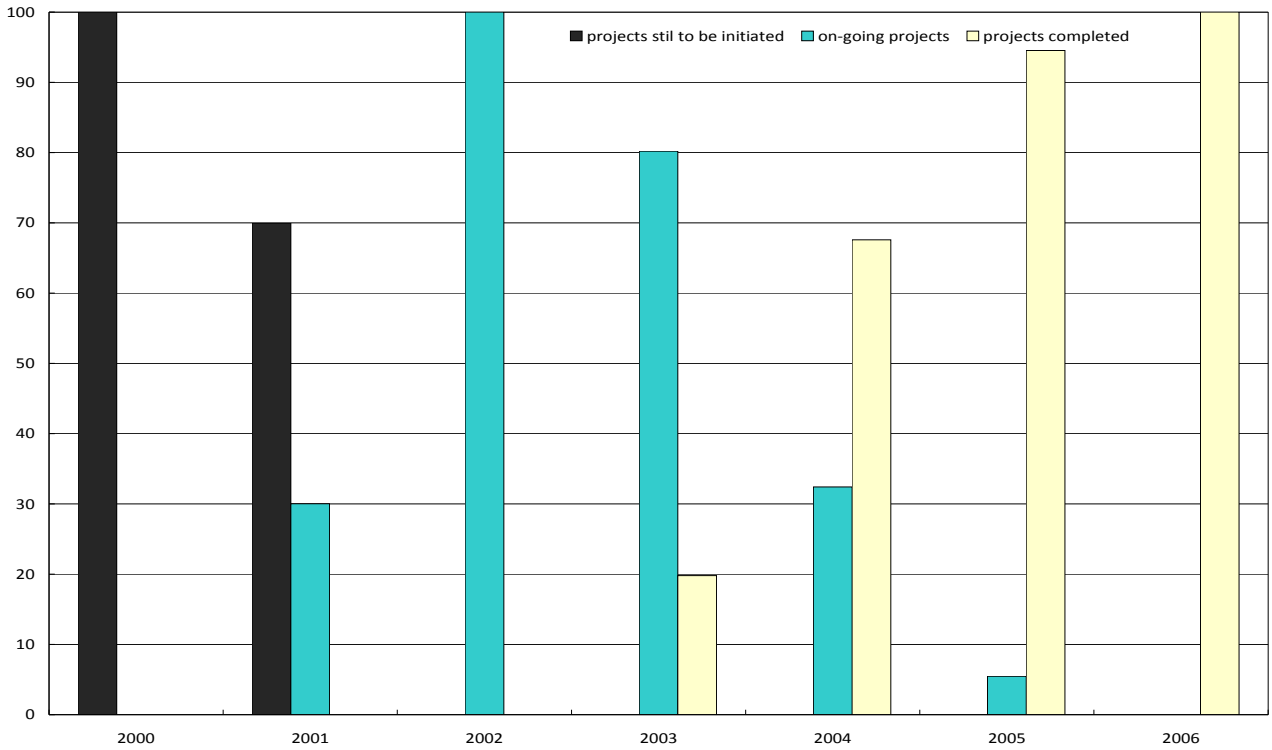


Fig. 3

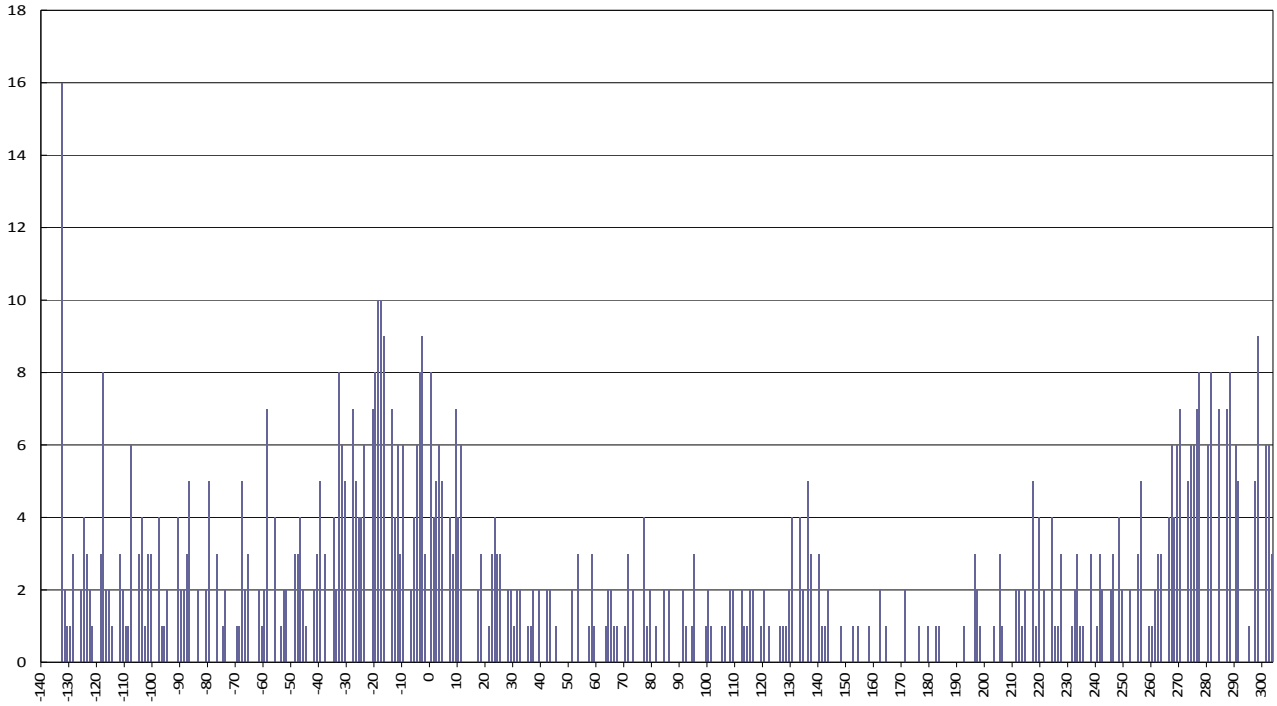
Actual Timeline of Subsidized Investments



Source: Ministry of Economic Development.

Fig. 4

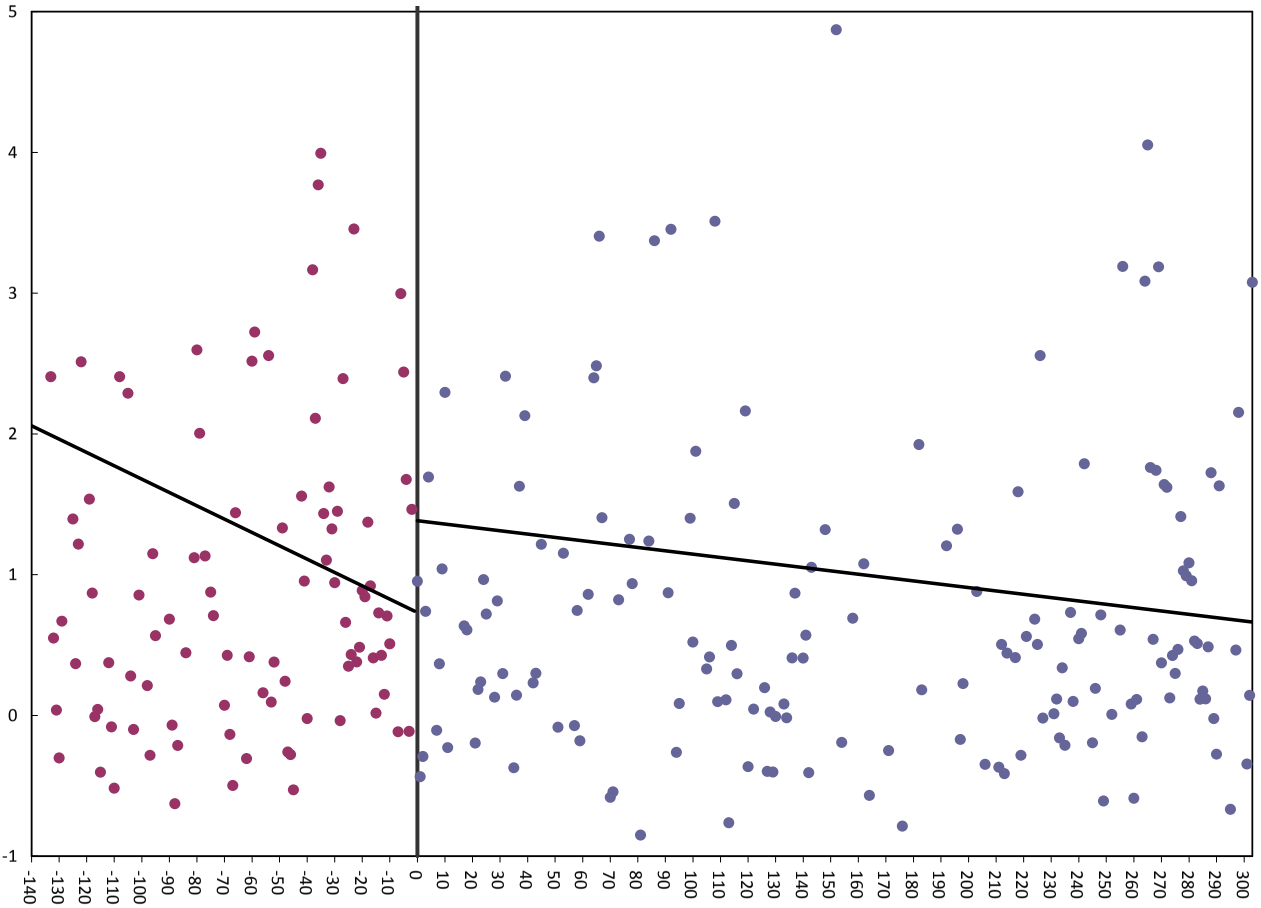
Frequency Distribution of Applications by Date



Source: Ministry of Economic Development and Cerved. Notes: the x axis shows the number of days before or after the suspension (day 0) of the assessment of the applications.

Fig. 5

The effect of FTI Subsidies on Investments



Source: Ministry of Economic Development and Cerved.

Table 1

Mean Differences in the Pre-treatment Year

	full sample				sample restricted to 90 days around cutoff			
	mean treated	mean controls	mean difference	p value	mean treated	mean controls	mean difference	p value
Net overall investments over capital	9,5942	9,4847	0,1095	0,24	9,5717	9,4682	0,1036	0,46
Net intangible investments over intangible capital	9,5102	9,3927	0,1175	0,22	9,4948	9,3829	0,1119	0,44
log(Sales)	0,6477	0,4110	0,2367	0,37	0,7054	0,4503	0,2551	0,64
log(Assets)	0,7518	1,0336	-0,2818	0,28	0,7079	0,7840	-0,0761	0,81
Long term debts over assets	0,1128	0,1047	0,0081	0,29	0,1149	0,1049	0,0100	0,39
Cash flow over assets	0,0892	0,0883	0,0009	0,84	0,0853	0,0892	-0,0038	0,56
Average debt cost	0,0383	0,0338	0,0044	0,05	0,0404	0,0330	0,0074	0,08
ROA	0,0819	0,0781	0,0038	0,46	0,0775	0,0774	0,0001	0,98
number of observations	329	422			239	126		

Source: Ministry of Economic Development and Cerved. Notes: all variables are referred to the pre-treatment year (2000) except capital and intangible capital (referred to 1999).

Table 2

SUR Estimates of Discontinuity of Covariates

	(1) full sample	(2) sample restricted to 90 days around cutoff
Net overall investments over capital	0,1126	-0,0953
	(0,1531)	(0,2189)
Net intangible investments over intangible capital	0,1488	-0,0244
	(0,1579)	(0,2242)
log(Sales)	0,3547	0,7914
	(0,4343)	(0,8388)
log(Assets)	-0,1641	0,0262
	(0,4311)	(0,4920)
Long term debts over assets	0,0165	0,0071
	(0,0128)	(0,0182)
Cash flow over assets	-0,0072	-0,0121
	(0,0072)	(0,0102)
Average debt cost	0,0083**	0,0099
	(0,0037)	(0,0064)
ROA	-0,0028	0,0016
	(0,0086)	(0,0117)
χ^2	7,63	6,67
p-value	0,47	0,57
number of observations	751	365

Source: Ministry of Economic Development and Cerved. Notes: all variables are referred to the pre-treatment year (2000) except capital and intangible capital (referred to 1999). The standard errors, clustered by technological level of the sectors (OECD definition), are reported in brackets. The number of asterisks shows the statistical significance of the coefficient: * 90%; ** 95%; *** 99%.

Table 3

The Effect of FTI Subsidies on Investments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment effect	0,3569	-0,2082	0,1463	0,2996	0,5482	-0,2458	-0,0759	-0,1115	-0,1946
	(0,3038)	(0,1901)	(0,9533)	(0,7554)	(0,8462)	(0,6482)	(0,1982)	(0,0901)	(1,3174)
Polynomial order	0	1	2	3	1		1	1	1
Bandwidth	∞	∞	∞	∞	90 days	12 days	∞	∞	∞
Control variables	no	no	no	no	no	no	yes	no	no
Dependent variable	Overall investments	Overall investments	Overall investments	Overall investments	Overall investments	Overall investments	Overall investments	Overall investments	Intangible investments
Period	2001-2007	2001-2007	2001-2007	2001-2007	2001-2007	2001-2007	2001-2007	2001-2006	2001-2007
R^2	0,003	0,010	0,012	0,013	0,017		0,052	0,011	0,007
number of observations	751	751	751	751	365	751	751	751	751

Source: Ministry of Economic Development and Cerved. Notes: all variables are referred to the shown period except capital and intangible capital (referred to 1999). The standard errors are reported in brackets; in column (6) standard errors are calculated by bootstrap; in the other columns are clustered by technological level (OECD definition). The number of asterisks shows the statistical significance of the coefficient: * 90%; ** 95%; *** 99%.

Table 4

The Effect of FTI Subsidies on Alternative Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	log(Sales)	log(Assets)	Long term debts over assets	Cash flow over assets	Average debt cost	ROA
Treatment effect	0,0388	0,1200**	-0,0086	-0,0024	0,0021	-4,7925
	(0,0620)	(0,0603)	(0,0119)	(0,0034)	(0,0028)	(3,3395)
Polynomial order	1	1	1	1	1	1
Bandwidth	∞	∞	∞	∞	∞	∞
Control variables	no	no	no	no	no	no
Period	2001-2007	2001-2007	2001-2007	2001-2007	2001-2007	2001-2007
R^2	0,0020	0,0044	0,0108	0,0012	0,0037	0,0037
Number of observations	751	751	751	751	751	751

Source: Ministry of Economic Development and Cerved. Notes: all variables are referred to the shown period except capital and intangible capital (referred to 1999). The standard errors, clustered by technological level (OECD definition), are reported in brackets. The number of asterisks shows the statistical significance of the coefficient: * 90%; ** 95%; *** 99%.

Table 5

The Effect of FTI Subsidies: Subsamples

	(1) small and medium firms sample	(2) high capital cost firms sample	(3) immaterial assets intensive firms sample
Treatment effect	-0,0720	-0,2008	-0,2649
	(0,1473)	(0,4125)	(0,4746)
Polynomial order	1	1	1
Bandwidth	∞	∞	∞
Control variables	no	no	no
Dependent variable	Overall investments	Overall investments	Overall investments
Period	2001-2007	2001-2007	2001-2007
R^2	0,0199	0,0007	0,0082
Observations	533	368	386

Source: Ministry of Economic Development and Cerved. Balanced panel. Notes: all variables are referred to the shown period except capital and intangible capital (referred to 1999). The standard errors, clustered by technological level (OECD definition), are reported in brackets. The number of asterisks shows the statistical significance of the coefficient: * 90%; ** 95%; *** 99%.

References

Angrist, J. D., & Lavy, V., "Using Maimonides' Rule to Estimate The Effect of Class Size on Scholastic Achievement", *The Quarterly Journal of Economics*, 114(2), pp. 533-575, 1999

Arrow, K. J., "Economic Welfare and the Allocation of Resources for Invention", in Nelson, R. R. (ed.), *The Rate and Direction of Inventive Activity*, Princeton University Press, 1962

Black, S. E., "Do Better Schools Matter? Parental Valuation of Elementary Education", *The Quarterly Journal of Economics*, 114(2), pp. 577-599, 1999

Bronzini, R., & de Blasio, G., "Evaluating the Impact of Investments Incentives: the Case of Italy's Law 488/1992", *Journal of Urban Economics*, 60(2), pp. 327-349, 2006

David, P. A., Hall, B. H., & Toole, A. A., "Is Public R&D a Complement or Substitute for Private R&D? A Review of the Econometric Evidence", *Research Policy*, 29(4-5), pp. 497-529, 2000

Duflo, E., Glennerster, R., Kremer, M., *Using Randomization in Development Economics Research: A Toolkit*, CEPR Discussion Paper 2059, 2007

Fan, J., & Gijbels, J., *Local Polynomial Modelling and Its Applications*, Chapman & Hall, 1996

Guiso, L., "High-tech Firms and Credit Rationing", *Journal of Economic Behavior & Organization*, 35(1), pp. 39-59, 1998

Hahn, J., Todd, P., & van der Klaauw, W., "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design", *Econometrica*, 69(1), pp. 201-209, 2001

Imbens, G. & Rubin, D., *Causal Inference in Statistics, and in the Social and Biomedical Sciences*, Cambridge University Press, forthcoming

Jaffe, A. B., "Building Program Evaluation into the Design of Public Research-Support Programmes", *Oxford Review of Economic Policy*, 18(1), pp. 22-34, 2002

Lach, S., "Do R&D Subsidies Stimulate or Displace Private R&D? Evidence from Israel", *The Journal of Industrial Economics*, 50(4), pp. 369-390, 2002

Lalive, R., "How Do Extended Benefits Affect Unemployment Duration? A Regression Discontinuity Approach", *Journal of Econometrics*, 142(2), pp. 785-806, 2008

Lee, D. S., "Randomized Experiments from Non-Random Selection in US House Elections", *Journal of Econometrics*, 142(2), pp. 675-697, 2008

Lee, D. S., & Lemieux, T., *Regression Discontinuity Designs in Economics*, NBER Working Paper 14723, 2009

McCrary, "Manipulation of the Running Variable in the Regression Discontinuity Design: a Density Test", *Journal of Econometrics*, 142(2), pp. 698-714, 2008

Merito, M., Giannangeli, S., & Bonaccorsi, A., "L'impatto degli Incentivi Pubblici per la R&S sull'Attività delle Pmi", in De Blasio, G., & Lotti, F. (eds.), *La Valutazione degli Aiuti alle Imprese*, Il Mulino, 2008

OECD, *Frascati Manual: Proposed Standard Practice for Surveys on Research and Experimental Development*, 2002

Pagan, A., & Ullah, A., *Nonparametric Econometrics*, Cambridge University Press, 1999

Pisoni, P., Bava, F., Busso, D., & Devalle, A., *Bilancio 2008*, Libro MAP, 2009

Silverman, B., *Density Estimation for Statistics and Data Analysis*, Chapman and Hall, 1986

Thistlewaite, D., & Campbell, D., "Regression-discontinuity Analysis: an Alternative to the Ex-post Facto Experiment", *Journal of Educational Psychology*, 51, pp. 309-317, 1960

Van der Klaauw, W., “Estimating the Effect of Financial Aid Offers on College Enrollment: A Regression-Discontinuity Approach”, *International Economic Review*, 43(4), pp. 1249-1287, 2002